



# **AN ANALYSIS OF THOMAS. KUHN'S NOTION OF INCOMMENSURABILITY**

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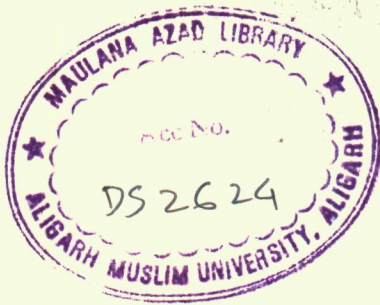
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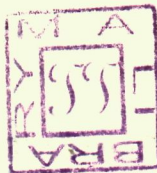
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**CERTIFICATE**

This is to certify that the dissertation entitled *an analysis of* Thomas' Kuhn's Notion of Incommensurability" is the research work carried by Mr. Quaisar Shakeel under my supervision in this Department.

The candidate has fulfilled the prescribed requirements of attendance and has also passed the pre requisit examination.

I certify that this dissertation is fit to be submitted for M.Phil degree.

A handwritten signature in black ink, appearing to be "M. Muqim", written over a horizontal line.

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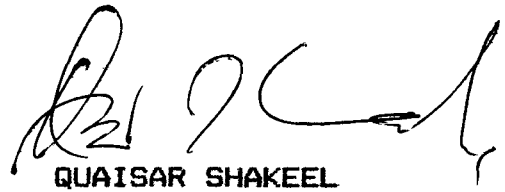
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## **CHAPTER I**

### **INTRODUCTION**

## CHAPTER I

### INTRODUCTION

The lexical meaning of incommensurability is generally given as having no common basis for comparison<sup>1</sup>. However in Philosophy of Science the term incommensurability is being used in the context of scientific theories that do not share meaning and hence they consequently can not be compared. A change of a theory necessitates a change in meaning of scientific terms that is, the sense of a term depends upon the theory in which that term occurs. In other words the meaning of a term is primarily theory laden.

The term incommensurability is however extendable to other areas such as culture, value system etc. Many social scientists held this view that one civilization/culture is no way better than other one. There is no common ingredients in making comparison between two or more competing cultures or civilizations<sup>2</sup>. Nevertheless our concern in this dissertation is to understand the notion of incommensurability in the context of science.



The notion of incommensurability was first introduced by Thomas Kuhn<sup>3</sup> and by Paul Feyerabend<sup>4</sup> independently that scientific theories are incommensurable keeping in view the historical development of Science. However, the scope of the notion of incommensurability is markedly different in the writing of Kuhn and Feyerabend.

Kuhn holds that change of meaning of scientific term occurs where there is a shift in paradigm while Feyerabend holds that the meaning change surely occurs when there is a shift in theory.

Many Philosophers of science consider that the term incommensurability, in both Kuhn's and Feyerabend's work, is used in different senses but in our view, the sense of incommensurability is the same, however they differ in scope. Feyerabend is so radical that he holds that scientific theories are necessarily incommensurable. Kuhn's position with respect to notion of incommensurability seems moderate in comparison to Feyerabend. In this dissertation we shall mainly focus on Kuhn's position.

Now we shall shed light on the two major approaches to the problem of incommensurability.

a). Logical positivists hold that the meaning of a scientific term remains constant across the scientific theories. For them the change in a theory does not implicate a change in meaning of scientific terms.

Firstly, the scientific terms share the same meaning if they occur in many scientific theories or paradigms. In other words the meaning of a scientific term is fixed and commensurable and does not change with a change of a theory they argue, for a meaningful comparison of competing scientific theories that the meaning of scientific term remains the same in theory shift. If a scientific theory acquires a different meaning in different theories then it would not be possible to compare scientific theories and crisis of theory choice may not be avoided. In order to eliminate such a possibility the logical positivists insist on the doctrine that the meaning of scientific terms is invariant.

Secondly, logical positivists argue that growth of scientific enterprise or progress in science should be understood only in terms of evolution of science. In addition, the progress of science may only be characterized in term of ability of a scientific theory in resolving scientific problems. For logical positivists, it is necessary to keep the meaning of scientific terms intact in order to appreciate the proper role a scientific theory plays in tackling scientific issues.

The logical positivists approach about commensurability seems to be compatible with their view about the nature of scientific knowledge and its progress. They consider scientific knowledge as accumulative and evolutionary over a period of time. They observe that accumulation of knowledge may not have sense if the meaning of a scientific term does not remain constant during a change of scientific theories.

b). Now we consider the tradition of new philosophy of science which has been emerged as a major alternative to the logical positivism which basically relies upon a study of

the history of science. Scientific episodes are being understood by these philosophers as paradigm cases for establishing that the scientific progress is characterized by radical meaning variance.

They reject the fundamental assumption of positivism that meaning remains constant from theory to theory. In other words, the meaning of a scientific term changes with a change in scientific theory or a theory is being modified or replaced by another theory in which that terms occur. Kuhn argues that the meaning of scientific term such as 'mass', 'velocity', 'momentum' etc., are having distinct meaning in classical Newtonian Physics and Einstein's mechanics. For example the 'mass' is absolute in classical mechanics while 'mass' is relative in latter case.

In the light of the above explanation the meaning of a term in a theory is supposed to be entirely dependent upon that theory.

According to Feyerabend, the description of every single fact is dependent on some theory. The meaning of every term

we use depends upon the theoretical framework in which it occurs. Words do not mean something in isolation, they attain their meaning by being a part of theoretical system. Moreover, when a new theory emerges to replace the old one the terms involved in that theory will change in such a way that there will be an elimination of the old meaning and the same term, although employed in both cases, will express two different and incommensurable concepts.

The thought that theories are incommensurable is the thought that theories simply cannot be compared and consequently there cannot be any rationally justifiable thinking that one theory is better than another. The problem is supposed to be that, since theories are incommensurable we cannot justify a preference for one over the other. However if theories are genuinely incommensurable why one should be faced the problem of choosing between them?

Lauden's proposal of problem solving effectiveness of a scientific theory may be regarded as a criterion for theory choice<sup>5</sup>. Similarly, Arthur Fine's proposal that the facts of reference may determine the meaning of scientific terms is worth mentioning<sup>6</sup>.

According to Kuhn, the prevalent normal scientific tradition which is not capable to cope with existing anomalies demands a scientific revolution which is not only incompatible but often incommensurable with existing theories.

In his book The Structure of Scientific Revolution, Kuhn continues to hold that the language of theories like classical mechanics and Phlogiston chemistry are untranslatable into the conceptual scheme that replaced them those of special relativity and of Oxygen chemistry. The inference seems to be that the problem of incommensurability in its turn a problem of non-translatibility.

Kuhn's approach is that meaning variance, conceptual change and conceptual growth are parts and parcel of the scientific enterprise and concludes that scientific change is revolutionary in nature.

So in the light of Kuhn's work and its subsequent receptions, two scientific paradigms or conceptual frameworks may be said to be incommensurable if there is no adequate translation from the language of one into the

language of the other. The shift in a scientific paradigm is the main cause for meaning change. In view of two distinct approaches about incommensurability, our aim is to evaluate and analyse the plausibility of the notion of incommensurability.

However, a systematic appraisal of historical development of philosophy of science would be useful in order to see the problem in correct perspective. The next chapter provides such an appraisal.

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## CHAPTER II

### LOGICAL POSITIVISTS APPROACH TO SCIENCE

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### LOGICAL POSITIVISTS APPROACH TO SCIENCE

To understand Kuhn's Notion of incommensurability and where the strength of his argument lies, it is necessary to understand the tradition in philosophy of science out of which his view has emerged. It is the purpose of this chapter and the following chapters to explore the background issues relevant in evaluating the notion of incommensurability.

There are two aspects of scientific theories that are of primary importance in any discussion of incommensurability. Our goal is to explain how competing scientific theories may be objectively compared, that is explaining in what sense a scientific knowledge has an objective basis, and explaining what sort of objective measures might be used for a comparison of contemporary theories. This is an issue concerning the epistemology of science. There is also a comparative task explaining how the expression of theories and their results may be correlated with one another. This

is an issue concerning the language of science. Philosophers of science of logical positivist tradition maintained that the epistemic status of scientific theories was dependent on the epistemic status of observational statements i.e. statements containing observational terms. Moreover there is assumption that the epistemic status of observation statements was unproblematic. If Kuhn has disagreement with this view about the epistemic status of scientific theories, it is that he challenges this assumption. These philosophers of science had first attempted to analyze the meaning of theoretical terms by explicit definitions in observational terms. When this method failed, related approaches were attempted such as Bridgman's operationalism, also with the little success. The philosophers conceded that there was a move to the meaning of the language of a theory by a set of explicit definitions. Positivists took as basic to their work in philosophy of science, the scientific theory as an abstract entity. The theory was conceived primarily as a set of universal generalization, axioms and theorems with predictive deductions. Kuhn and his contemporaries took, as basic to their work in philosophy of science, the scientific paradigm as a social structure. The scientific community was

viewed primarily as a group of individuals tied together by a shared interest in certain problems and shared agreement about certain solutions. The basic difference between the positivist's thought and Fuhn's thought is about the difference in scientific perspective.

Positivists thought that what was most important about the language of science could be discussed through talk about explicit definitions. Fuhn maintains that what is most important about the language of science is that which escapes explicit definitions. Positivists seem to think that the most important parts of the epistemology of science can be analysed by a set of logical relations. Fuhn, on the other hand, views the epistemology of science as a context dependent matter to be determined by standards shared by scientific community. There are differences which are crucial for an understanding of Fuhn's notion of science.

Rudolf Carnap is the most important representative of logical positivism. His view about the nature of science is most important which he labelled Scientific Empiricism. Carnap's Philosophy of science was the best known and is the

rigorously worked out. We shall also consider alternative positivist approaches to Philosophy of science but not in detail.

### Carnap on the Nature of Theories and Empirical Meaning:

In 1935 we find Carnap, in The Rejection of Metaphysics making the following methodological claim about one of the tasks of philosophy:

The function of logical analysis is to analyse all knowledge, all assertions of science and of every day life. In order to make clear the sense of each such assertion and the connections between them. One of the principal task of the logical analysis of a given statement is to find out the method of verification for that statement. The question is, what reasons can there be to assert this statements, or, How can we become certain as to its truth or falsehood? This question is called by the Philosophers

the epistemological question, epistemology or the philosophical theory of knowledge is nothing other than a special part of logical analysis<sup>1</sup>.

This question is an good exposition of the approach and the focus of scientific empiricism. The first thing is to note is the hope that Carnap expresses for logical analysis. Epistemology itself is supposed to be nothing other than analysing certain features about the logical structure of statements and the logical relations that obtain among statements. Carnap sees a significant role for the method of verification for a statement. What is implied here, is his belief that we cannot have a reason to assert a statement that is not verifiable. This is the focus of Carnap's work. Thus we may say, this much about the epistemology of science according to scientific empiricism: a necessary condition for a statement to being an objective knowledge claim is that it be verifiable. This point needs to be stressed because, while philosophers are generally familiar with the positivist verification thesis, it is generally viewed as only a point about the meaning of a term or statement. What is easily overlooked is that, according to scientific

empiricism, a statement that is not verifiable is one that we could not have a reason to believe.

The theory of verification also bears in the obvious way on the language of science. In order to know how to correlate expressions of a theory and its results, we must have an analysis for the meaning of statements in a theory. Thus the problem of verification, and establishing a criterion of verification is central to the scientific empiricist approach to the notion of incommensurability. As the title, The Rejection of Metaphysics suggests, Carnap wants, among other things, to show that all so called metaphysical statements are not verifiable and hence cannot have any claim to knowledge. Carnap says in this way:

It will call metaphysical all those statements which claim to represent knowledge about something which is over or beyond all experience, e.g. about the real essence of things, about things in themselves, the absolute and such like<sup>2</sup>.

Thus one problem that Carnap and other positivists faced was that it was forever a test of proposed verification criteria whether or not statements such as those mentioned by Carnap above turned out to be verifiable. It was not enough to understand the nature of science, metaphysics had to be ruled out also. There was a general agreement that mere observational reports were empirically meaningful and unproblematic. The problem was to show how scientific theories, which made reference to unobservable entities, had significance in a way in which other disciplines such as religion or speculative metaphysics which made reference to unobservable entities, did not have significance.

Carnap's initial view may be summarized as:

He considers the observational language to be a set of phenomenological reports about one's present sensation states. The theoretical language includes a set of mathematical and logical principles (meaningful because they are analytic) and a set of theoretical generalizations will be empirically meaningful if and only if truth conditions for those generalizations can be explicitly given in the



observation language by means of correspondence rules that connect theoretical statements with observational statements.

First, if the observational language is characterized as a language of sensation, then it becomes essentially private. Intersubjective discourse becomes impossible and it is coincidence that we all speak the same language, if we in fact do. Carnap thus abandons the phenomenal approach to the observational language what he calls a physicalist approach. The observational language is characterized in terms of readily observable properties of ordinary, physical object. Carnap's usual form of an observational statement is then  $F(a,b,c,d)$  where  $a, b$  and  $c$  are spatial coordinates,  $d$  is a measure of time and  $F$  is some observable property say red. This statement would then be read as At time  $d$ , location  $a, b, c$  is red.

Second, if truth conditions for every theoretical statement can be given in terms of the observation statements, it is difficult to see what purpose the theory serves. If the entire theory could be restructured in terms of its

observational equivalent, then that set of observational statements could serve as a prediction of future observations. The theory itself seems superfluous and dispensable.

Finally, Carnap realizes that not all theoretical terms are amenable to the sort of explicit definition he initially requires. First, any universally quantified statement has an infinite number of observational consequences, and hence does not have a finitely statable observational equivalent. But furthermore, dispositional terms present a special problem. Consider a definition of a term like "Soluble" i.e. 'x is soluble is equivalent to: If x is placed in water then x dissolves seems inadequate, because an object never placed in water would be soluble according to this definition and this is not what we mean by soluble. Other attempts to provide an explicit definition for "soluble" prove equally inadequate. Thus Carnap concludes that there are certain theoretical terms, in particular dispositional terms which can only be partially defined. The early view of logical positivists is still instructive because it provides a guideline for basic analysis that all subsequent accounts

in the positivist tradition would try to follow. A theory is to be divided into two parts, its set of theoretical laws on the one hand, and its observational consequences on the other hand. These are to be related by connecting principles which define the theoretical terms in observational terms. Furthermore, if we take observation statements to be in some veridical, then the observational consequences of the theory provide some measure of correspondence to the truth for the theory. Thus Carnap's early view provides some guidelines about discussing the epistemology of science. Theoretical statements without observational import are to be shunned as not meeting the requirement for being a claim to knowledge, if there is to be one, is in terms of predictive consequences of two theories share an observation language, then the observation language can mediate to allow comparison of theoretical terms. It may even be that certain terms, like "mass" or "velocity", will be defined in the same way in different theories allowing a direct comparison.

In "Testability and Meaning" Carnap develops his revised criterion. He says:

The connection between meaning and confirmation has sometimes been formulated by the thesis that a sentence is meaningful if and only if it is verifiable and that its meaning is the method of its verification. But from our present point of view, this formulation although acceptable as a first approximation is not quite correct. But its over simplification, it led to a too narrow restriction of scientific language, excluding not only metaphysical sentences but also certain scientific sentences having factual meaning<sup>3</sup>.

Carnap defines empirical meaning entirely in terms of confirmation. Confirmation is defined as follows:

A sentence S is called confirmable.... If the confirmation of S is reducible to that of a class of observable predicates ..... A predicate P is called confirmable....if P is reducible .... to a class of observable predicates<sup>4</sup>.

Now let us look at Carnap's definition of empirical meaning:  
Requirement of Confirmability (RC): Every synthetic sentence must be confirmable<sup>5</sup>. Carnap says of this requirement.

RC ..... Suffices to exclude all sentences of a non-empirical nature, e.g., those of transcendental metaphysics in as much as they are not confirmable, not even incompletely. Therefore it seems to me that RC suffices as a formulation of the principle of empiricism; In other words, if a scientist chooses any language fulfilling this requirement no objection can be raised against this choice from the point of view of empiricism<sup>6</sup>.

The problem of distinguishing observational and theoretical aspects of language will be of primary importance. So let us consider how Carnap accounts for this distinction in order to establish the notion of confirmation which supports such a powerful conclusion. What Carnap offers is an explanation:

A predicate "F" of a language L is called observable for an organism (e.g. a person) N, if, for suitable arguments e.g. "b", N is able under suitable circumstances to come to a decision with the help of few observations about a full sentence say  $P(b)$  i.e. to a confirmation of either ' $P(b)$ ' or ' $\sim P(b)$ '.<sup>7</sup>

However Carnap says nothing about the force of the word "suitable" in this characterization, nor does he say anything about how many observations make up the decision process that results in a confirmation.

Carnap is aware of these short comings:

Suppose a sentence "S" is given, some test observations for it have been made and "S" is confirmed by them in a certain degree. Then it is a matter of practical decision whether we will consider that degree as high enough for our acceptance of S. There is no general rule to determine our decision. Thus the acceptance and the rejection of a (synthetic) sentence always contains a conventional component<sup>8</sup>.

Moreover Carnap talks about explanation of observation itself:

This explanation is necessarily vague. There is no sharpline between observable and non-observable predicates because a person will be more or less able to decide a certain sentence quickly i.e. he will be inclined after a certain period of observation to accept the sentence<sup>9</sup>.

This characterization will not be sufficient to reject metaphysics if observation is a matter of convention. There could certainly be a community of language users that would adopt the convention of considering certain sentences we regard as highly speculative, highly confirmed after a few observations. For example, a group of individual might see the hand of God in everything around them and thus conclude after a few observations that God exists. Thus for this language group "God exists" would be a highly confirmed, empirically meaningful statement according to Carnap's own view despite the fact that this is precisely the sort of proposition that Carnap wishes to reject as metaphysical and meaningless.

Hence, Carnap has in no way addressed the epistemic status of observation terms. It is clear from the structure of science he proposes that the epistemic status of theories will depend on the epistemic status of observation statements used in the definition and predictions of the theory. As he thought that his doctrine i.e. observational statements have an unproblematic objective basis was sufficient for the rejection of metaphysics but we may conclude that this remains an assumption on Carnap's part.



### The Later Phase of Carnap

In the methodological character of theoretical concept Carnap attempts to provide a revised and weakened criterion for, what he here calls cognitive significance because he viewed  $R_c$  (Requirement of confirmability) as too strong requirement that even ruled out some of the proposition of science that he thought should properly be considered as meaningful.

The basic structure of Carnap's system is still much the same. He still distinguishes, within a given language  $L$ , between the theoretical language  $L_T$ , and the observational language  $L_O$ . These two parts of  $L$  are connected by correspondence rules (or simply C- rules) which provide definitions and partial definition for the theoretical terms in terms of the observation language.

### Bridgman's Operationism:

Carnap never developed a rigorous account of observation, nor did he seem to think that such an account was necessary. Thus

at the out set of "The Methodological Character of Theoretical Concepts" we find Carnap saying, "I shall leave aside the problem of a criterion of significance for the observation language, because there seem to be hardly any points of serious disagreement among philosophers today<sup>10</sup>."

Most Philosophers in the positivist tradition seemed to agree with Carnap in this respect; there were few attempts to say anything more rigorous about observation than what Carnap had offered, one notable exception was the operationist tradition, first expounded by the physicist P.W. Bridgman in The Logic of Modern Physics. Operationism was not a position opposed to Carnap. Rather, it was an attempt to explore more seriously the relation between a theory and the data it purports to explain, and an attempt to offer a slightly different account of empirical meaning. Bridgman took the basic linguistic units empirical meaning to be predicates, not sentences, and this has a weakening effect on the criterion (as noted earlier). However Bridgman still hoped to give theoretical terms some kind of complete definition. Complete definition was a stronger requirement than the positivists account.

The basic operationist's thesis is that a theoretical term is to be defined as the set of measurement operations which must be performed in order to make a judgement ascribing that term to some object or system.

The Bridgman offers as an example for the concept of length:

We may illustrate by considering the concept of length. What do we mean by the length of an object? We evidently know what we mean by length if we can tell what the length of any and every physical object is and for the physicist nothing more is required. To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed that is, the concept of length involves as much as and nothing more than a set of operations<sup>11</sup>.

Thus length is synonymous with a set of measuring operations which produce a measurement of length, and for length to be an empirically meaningful concept there must be such an operational definition. In general, Bridgman says:

The concept is synonymous with the corresponding set of operations. If the concept is physical, as of length, the operations are actual physical operations, namely, those by which length is measured, or if the concept is mental, namely those by which we determine whether a given aggregate of magnitude is continuous<sup>12</sup>.

There are several advantages to this approach. First, Bridgman does not need to distinguish between the analytic meaning of mathematics and logic, and the empirical meaning of science, as Carnap does. Rather, all of these are operationally meaningful, only the mechanism of the operations may differ. Second, Bridgman implicitly provides a more comprehensive account of observation. A observation report is the report of the result of performing some operations, reading the scale on a balance, counting the

lines on a spectograph and so on. There is still a ambiguity, because operationism says little about what counts as a physical operation. There may be several ways to measure what we generally regard as the same property. Thus we may take a measuring rod, and by laying it to end to end, determine the length of some object. But we may use a totally different procedure for determining length. Using a spectroscope we might determine the velocity of the object from its red shift, measure the amount of time it takes the object to pass a particular point, and use these to calculate its length. Both these procedures are legitimate calculation of something that is "length" and yet the procedures have nothing in common. Thus Bridgman says:

In principle the operations by which length is measured should be uniquely specified. If we have more than one set of operations, we have more than one concept, and strictly there should be a separate name to correspond to each different set of operations<sup>17</sup>.

According to Bridgman, each unique set of operations is synonymous with a different concept. In other words, there

are never two different ways of measuring the same property. This is not an easily acceptable determined way of measuring. We intend to think that length is one single property of an object, regardless of how it is measured, or at least to think that there is more than one way to measure length. Furthermore, the assumption seems to be crucial to scientists. It is by corroboration between different means of measuring a single property that scientists often gain the most useful information about the accuracy of measuring devices.

Another consequence of the operationist insistence on strict synonymy between concept and operation is that an object does not possess a certain property at any time when the operation which measures that property is not being performed. As Carl Hempel has pointed out, we think that objects have mass, for example, even at times when we are not measuring their mass. However;

An operational definition of a concept will have to be understood as ascribing the concept to all those cases that would exhibit the characteristic response if the test conditions should be realized. A concept thus characterised is clearly not synonymous with the corresponding set of operation<sup>14</sup>.

If the operationists wish to maintain the thesis of strict synonymy, then we can not say that an object has mass when we are not measuring its mass. But the requirement of synonymy is one that Bridgman, seems unwilling to give up and it is one of the principal differences between operationism and other positivist account of empirical meaning.

#### Hempel's Philosophy of Science

Carl Hempel's own views on empirical meaning began to diverge from Carnap's in the fifties, at about the time Carnap published "The Methodological Character of Theoretical Concepts".

Hempel offered the most liberal requirements for empirical meaning. Hempel reasons that once we accept that we can not have a system of explicit definitions, operational or otherwise that give observational interpretation to the theoretical language, then there is no reason to restrict ourselves to Carnap type reduction sentences which provide interpretation for the sentences and predicates of the theoretical language individually and item by item. Hempel says:

A partial specification of the meaning of a set of nonobservational terms might be expressed, more generally, by one or more sentences that, connect those terms with the observational vocabulary but do not have the form of reduction sentences. And it seems well to countenance, for the same purpose, even stipulation expressed by sentences containing only nonobservational terms, for example, the stipulation that two theoretical concepts are mutually exclusive may be regarded as a limitation and in this sense, a partial specification of their meaning<sup>15</sup>.



Hempel argues here that sentences in the theoretical language do not necessarily get their interpretations in isolation and independent of one another. The interpretation of some sentences or predicate may be affected by a whole group of sentences, even a group of sentences that are themselves entirely nonobservational. Thus Hempel recommends:

Generally, then, a set of one or more theoretical terms,  $t_1, t_2, \dots, t_n$  might be introduced by any set  $M$  of sentences such that (i)  $M$  contains no extra logical terms other than  $t_1, t_2, \dots, t_n$  and observation terms (ii)  $M$  is logically consistent and (iii)  $M$  is not equivalent to a truth of formal logic  $\dots$ . A set  $M$  of this kind will be referred to briefly as an interpretive system, its elements as interpretive sentences<sup>16</sup>.

From this passage we see that Hempel advocates a view on which the interpretation of a theory is an interrelated whole and thus questions of empirical meaning for particular

predicates or for particular sentences make no sense. We can only ask "does the theory as a whole have empirical meaning?" It is interesting to note that this sort of holistic approach to the meaning of theories is most frequently attributed to Fuhner. It is not a view that scientific empiricists advocated happily, but one that Hempel at least was willing to accept. Hempel points out another, consequence of his view:

There remains no satisfactory general way of dividing all conceivable systems of theoretical terms into two classes; those that are scientifically significant and those that are not, those that have experimental import and those that lack it. Rather, experimental or operational, significance appears as capable of gradations<sup>17</sup>.

Hempel maintains that the goal of scientific empiricism is to provide a way determining the empirical significance of a theory. He is also aware, that he can not rule out theories which include some metaphysical statements as being

meaningful, since meaning is no longer assigned statement by statement<sup>18</sup>. However, this was not an acceptable position for many positivists.

#### Reichenbach on Probability and Meaning

Hans Reichenbach's analysis of empirical significance is quite similar to Carnap's position. There are two main differences. First, Reichenbach makes an explicit appeal to probability in his account of empirical significance. What Reichenbach calls weight is very much like Carnap's notion of confirmation, but Reichenbach is very direct about its probabilistic nature and the importance of probability for empirical significance.

Second, Reichenbach differs with Carnap significantly on his views about the foundations of probability. Discussion of this second point is not important here, but the first point merits some consideration.

Reichenbach first considers a position which was characteristic of early positivism. It is based on the following two principles:

1. A proposition has meaning if and only if it is verifiable as true or false.
2. Two propositions have the same meaning if they obtain the same determination as true or false by every possible observation<sup>19</sup>.

These requirements amount to a requirement of complete verifiability, i.e., there must be some set of possible observations that would conclusively demonstrate the truth or falsity of the proposition. These requirements also result in a claim very similar to operationism.

Reichenbach does not endorse this view, but he takes it as a starting point to develop his own position. He worries first about the notion of possibility expressed in the above

principles. He considers two possible interpretations, that of logical possibility, and that of physical possibility.

Thus, for example, to construct a bridge across the Atlantic is physically possible, but it is not possible to construct a perpetual motion machine, even though this is logically possible. To interpret "possibility" as physical possibility seems, according to Reichenbach, too restrictive, because certain seemingly meaningful sentences would turn out to be meaningless:

Take a sentence concerning the interior of the sun; that there are forty million degrees of heat in the sun's center cannot be verified because it is physically impossible to introduce an instrument of measurement into the sun's bulk<sup>20</sup>.

On the other hand, to interpret "possible" as logically possible seems to offer too broad an interpretation. It is logically possible that any number of things might be observable.

Reichenbach's response is to maintain the physical interpretation of possibility, but to introduce indirectly verifiable, as well as directly verifiable propositions.

Reichenbach notes that directly verifiable propositions will, be observation propositions, while indirectly verifiable propositions will generally be the theoretical claims of science. Reichenbach first proposes that a proposition is indirectly verifiable if its truth conditions are equivalent to a class of directly verifiable propositions. This suggestion is very similar to Carnap's stand at the time of the Vienna Circle. Reichenbach notes that the equivalence class for indirect propositions will often be an infinite set of propositions. It becomes a practical impossibility to actually verify an indirect proposition, since we are incapable of verifying an infinite set of directly verifiable propositions.

Reichenbach's resolution is to note that the theoretical principle is probable, given the observations. Thus Reichenbach offers the following principles as the final version of his position:

First principle of the probability theory of meaning: a proposition has meaning if it is possible to determine a weight, i.e. a degree of probability, for the proposition ... Second principle of the probability theory of meaning: two sentences have the same meaning if they obtain the same weight, or degree of probability, by every possible observation<sup>21</sup>.

The difficulties with Reichenbach's account are similar to those with Carnap's. We need an account of what observation is, and that account is problematic. Reichenbach on this point is more strict than Carnap. He insists on a phenomenological account of observation, largely because he foresees the sort of difficulties that Carnap's account of observation must face.

Reichenbach considers how we are to distinguish observational statements from theoretical statements. He considers whether or not the merely technical distinction between general and singular statements will suffice.

According to Reichenbach, this will not do.

Let us consider the Michelson experiment. Every physicist knows that the statement concerning the equality of the velocity of light in different directions is not directly observed in the Michelson experiment but that it is inferred .... Directly observed are images in telescopes or on photographic plates, or indications of thermometers, galvanometers, etc.<sup>22</sup>

Reichenbach's concern is this: in order for observation statements to do the work they have been assigned in his system, they must be absolutely verifiable. But, if the statements we need -- for example "the velocity of the speed of light is observed to be equal in all directions at time "t" for experiment "e" -- is arrival at only inferentially, then it is not absolutely verifiable. This seems to suggest that some more operational concept of observation would be better, but Reichenbach points out:



A statement concerning a physical fact, even if it concerns a simple fact of daily life, never refers to a single fact alone but always includes some predictions. If we say, "There is a table in my room, before my eyes, at 7.15 P.M." this contains the prediction ... "If I put a book on the table, it will not drop"<sup>23</sup>.

There is one other point about Reichenbach's view which draws our attention. Basic to his definition of meaning is a notion of physical possibility employed in defining possible observations. These possible observations will be used to determine whether or not, physical theories are meaningful. But if it is an open question which physical theories are meaningful until they have passed the test, then it is difficult to see where the notion of physical possibility comes from.

A notion of physical possibility is linked to apriori physical theory, it seems that the whole process will be circular: which physical theories are meaningful will be determined by what the physically possible observations are,

and what the physically possible observations are will depend on what the meaningful physical theories are.

### Nagel on Reductionism

So far, nothing has been said about the relations between two or more theories, or how one is to choose between rival theories, or when the replacement of one theory by another is rational and to be considered scientific progress. Positivism had very little to say on these questions, instead it addresses the question such as "Under what conditions is one theory reducible to another theory?" Three assumptions are implied by this question: (i) that when one theory is reducible to another, the change from the former to the latter is a rational change, and hence constitutes scientific progress, (ii) that such reduction actually do occur in the history of science, and (iii) that such reduction are identifiable and knowable in the history of science. Positivism never questioned any of these assumptions.

In the work of Ernest Nagel we find the clearest explication of the positivist analysis of theory reduction. Nagel writes:

The phenomenon of a relatively autonomous theory becoming absorbed by, or reduced to, some other more inclusive theory is an undeniable and recurrent feature of the history of modern science. There is every reason to suppose that such reduction will continue to take place in the future<sup>24</sup>.

Thus we see that Nagel simply assumes the claim that theory reduction is "an undeniable and recurrent feature" in the history of science. His concern is not to justify that this is the process by which science progresses, but rather his concern is to analyse an obvious feature of the history of science.

Nagel identifies two different ways in which theory reduction may occur, what he calls "homogeneous" and "heterogeneous" reduction. Homogeneous reduction occurs when

one theory about a particular type of phenomena is shown to be a branch of a broader theory about the same type of phenomena. A case of theory reduction is most readily identifiable as homogeneous when the descriptive predicates employed in the two theories are essentially the same. For example, Galileo's theory of the motion of falling bodies uses the same descriptive terms as Newton's mechanics (velocity, acceleration, distance, time, etc.), and was eventually subsumed as a subdivision of general Newtonian mechanics. Heterogeneous reduction occurs when one theory is reduced to another theory which concerns qualitatively different sorts of physical phenomena. Heterogeneous reduction is identifiable because of the disparity between the descriptive predicates of the two theories. Thus, at the end of the nineteenth century, thermodynamics was explained in terms of mechanics, despite the fact that "temperature" a thermodynamic concept, and "kinetic energy", a mechanical concept, are apparently disparate concepts.

It is a second kind of reduction Nagel is interested in. He considers the reduction of Galilean laws of motion to Newtonian mechanics to be straightforward and unproblematic.

Nagel gives a set of requirements for heterogeneous reduction that must be met in order to establish that one theory is reducible to another. Nagel first states three formal conditions that must be met. He says:

The axioms, special hypotheses, and experimental laws of the sciences involved in a reduction must be available as explicitly formulated statements, whose various constituent terms have meanings unambiguously fixed by codified rules of usage or by established procedures appropriate to each discipline. To the extent that this elementary requirement is not satisfied, it is hardly possible to decide with assurance whether one science (or branch of science) has in fact been reduced to another.<sup>25</sup>

Nagel's account of theory reduction faces some problems of not providing a complete account of the rationality of scientific change. It requires, that there be a predecessor theory with which to compare. Furthermore, not all

comparisons between theories are between predecessor and successor. There are times in the history of science when two theories rival one another to be the successor to some older third theory. Nagel's account says nothing about how such rival theories might be compared.

The logical positivists' criteria of meaning never succeeded either in explaining the meaning of theoretical terms or ruling out a class of statements, namely metaphysical, that scientific empiricists desired to show as not meeting the necessary conditions for objective knowledge claims. In order to do so we would require both further revisions of criteria of meaning and a richer account of observation statements.

The lesson is an instructive one, however. We see that some positivists, like Hempel, was not necessarily opposed to letting nonempirical statements play some role in science when conjoined with theories that otherwise have empirical contents. There is also recognition of the fact that theoretical terms are not and should not be amenable to complete, explicit definition. There is also some indication

that both Hempel and Reichenbach had an appreciation for the holistic aspect of the language of science.

Though a detailed analysis of observation statements was not forthcoming from scientific empiricism, this is more because there seemed to be no need for such an analysis than because (as with theoretical statements) such analysis proved to be intractable. The dynamics of theory change simply was not an issue. Only Nagel seriously addresses the concept of scientific progress, and his analysis focusses on the one narrow concept of theory reduction.

Scientific empiricists had to accept that a full analysis of the meaning of theoretical terms required more than a set of explicit definitions. It required that some meaning be imparted implicitly. Furthermore, there is at least speculation on the part of some scientific empiricists that meaning is holistic: theoretic terms do not get the full meaning independent of any other theoretic terms, but rather that meaning is a feature of the theory as a whole. The meaning of observation terms is neither discussed nor a concern.

The epistemic status of scientific theories according to scientific empiricism is simply one large question mark. It is implicit in the work of Carnap and others, and explicit in the work of Nagel, that how we judge the merit of a theory will depend on the observational consequences of the theory. This view, however, tells us nothing about the ultimate status of theories unless we have a view about the epistemic status of observation statements. On this latter point scientific empiricism is simply silent. There is at best an implicit assumption that observation statements constitute a privileged class that are in some sense self-justifying, but this position is never actually stated or argued for. Further, the only account of scientific progress, Nagel's depends entirely on the assumption that the observation language is fixed and reidentifiable from one theory to the next.



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## CHAPTER III

### KUHN'S RESPONSE TO LOGICAL POSITIVISM

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### Kuhn's Response to Logical Positivism

The main goal of logical positivism was to demonstrate that science has a privileged place in our knowledge, a kind of objectivity that other disciplines lack. Thus scientific empiricism focuses on the difference between science on the one hand, and metaphysics, religion, and mythology on the other hand.

The group of Philosophers that will be under discussion in this chapter have a rather different goal. This group of philosophers are known as new philosophers of science who give emphasis on the importance of historical context and do not view science as privileged one as far as epistemic content of science is concerned. Their focus is on the actual history of science, and how the actual practice of scientists reveals that the scientific enterprise is just as fraught with metaphysical beliefs, subjectivity, and human irrationality as any other discipline.

Paul Feyerabend even goes so far as to claim that this is the way that science should be conducted:

A scientist who is interested in maximal empirical content, and who wants to understand as many aspects of his theory as possible will accordingly adopt a pluralistic methodology, he will compare theories with other theories rather than with experience, data or facts, and he will try to improve rather than discard the views that appear to lose in the competition. For the alternatives, which he needs to keep the contest going, may be taken from the past as well. As a matter of fact, they may be taken from wherever one is able to find them from ancient myths and modern prejudices, from the lubrications of experts and from the fantasies of cranks. The whole history of a subject is utilized in the attempt to improve its most recent and most advanced stage. The separation between the history of a science, its philosophy and the science itself dissolves into thin air and so does the separation between science and non-science<sup>1</sup>.

My goal in this chapter is to identify the key issues in philosophy of science that lead Kuhn to provide a notion of incommensurability. Towards that end it will be necessary to identify what the real disputes are between logical positivism and new philosophers of science. Without an appreciation for their actual differences it will be impossible to understand the concept of incommensurability.

We will consider here the views of philosophers like Paul Feyerabend, Thomas Kuhn and others. We shall be concerned with their treatment of these questions: How are theoretical terms defined? What is the nature of scientific change?

One of the chief criticisms that Kuhn and others have raised against logical positivism is that the empiricist picture of the structure of scientific theories is unrealistic; it does not reflect the way in which science is actually done. Rather the task of constructing an accurate picture of the structure of scientific theories would seem to be a historical one. Historians should and do contribute to our understanding of the history of science.

Philosopher attempts to offer that the scientific theories should be judged by epistemic standards. If the philosopher offers such standards with no regard to the actual practise of science, then a constraint on the philosopher is to offer epistemic guidelines that a scientist actually could fulfill. If we are to know how to compare two scientific theories, we must know what it is, that we are going to compare.

We must have a criteria for identifying a scientific theory and a means of analysing it. Thus the question, "what is the structure of a scientific theory", becomes not just a historical question but philosophical.

The contextualists (new philosophers of science) answer to this question differs from the answer of logical positivism. It differs primarily because contextualists have a different answer to the problem of definition. Scientific empiricism makes two main assumption which characterize the structure of a scientific theory.

First, theoretical terms are to be defined ultimately in observational terms, and observational terms are not



themselves in need of definition. This has left scientific empiricists with the awkward consequence that, theoretical terms are not generally completely definable, in other words definition do not exhaust the meaning of theoretical terms. Second a theory is to be judged by its observational consequences. These two assumptions suggest the traditional hierarchical view of scientific theories: a set of laws of the theory, stated in the theoretical language, where theoretical terms are defined by bridge principles to the observation language, and where there are some observational statements that represent the predictions deducible from the laws of theory. Contextuation emphasis what is over looked by logical positivism. There must be more to the meaning of theoretical terms than what is stated in a set of partial definitions. The focus is on what Feyerabend calls "natural interpretations" and what Kuhn calls "exemplars". To make these terms clear, it will be useful to focus on some particular examples taken from the history of science. A transition in the hisotry of science discussed by both Feyerabend and Kuhn is the change from the ptolemaic geocentric view of astronomy to the copernican heliocentric view of astronomy.

A more complete picture and the role of natural interpretation may be seen in the structure of a scientific theory by turning to Kuhn's analysis of exemplars. We find that the term "exemplar" comes from Kuhn's later writings, and is an attempt on his part to clarify one of the meanings he associates with the term "Paradigm" in "The structure of Scientific Revolution".

Kuhn begins his explanation of exemplars by asking the question. How do scientists attach symbolic generalization to nature? Kuhn says:

Since the abandonment of hope for a sense-datum language, the usual answer to this question has been in terms of correspondence rules. These have already been taken to be either operational definitions of scientific terms or else a set of necessary and sufficient condition for the term's applicability. I do not myself doubt that the examination of a given scientific community would disclose

a number of such rules shared by its members. I do doubt that the correspondence rule discovered in this way would be nearly sufficient in number or force to account for the actual correlation between formalism and experiment made regularly and unproblematically by members of the group<sup>2</sup>.

Kuhn argues here that logical positivism has offered an unrealistic picture of science. He holds that a theory is related to natural phenomena by concrete and particular examples, which he calls exemplars. By such examples he means exactly the sort of examples that Feyerabend calls natural interpretations. The Ptolemaic seeing the falling stone as an elucidating instance of ptolemaic principles of motion, the Galilean seeing the ship on the sea as an instance of Galilean principles of motion. Now we shall see, what occurs when one theory replaces another<sup>3</sup>.

The theme of Kuhn's structure of scientific revolution is to explain the dynamics of theory change. He provides episodes

from history of science to show a pattern of theory change. Initially there is a scientific community struggling with a common set of problems. However, the community has no overall agreement about which theory is the correct theory, and no theory among available alternatives has demonstrated itself capable of solving the problem. This Kuhn calls the pre-paradigmatic stage. After a time, some theory will emerge that claims to solve number of key problems. This theory will have with it a paradigm accepted by those members of the scientific community and according to which that theory is the best theory. The paradigm will identify, among other things, the set of problems considered to be the proper domain of the theory. The theory will not solve all of them, but this will be the task of scientists in the community to work out the intended application of theory, Kuhn calls it the period of normal science.

After sometime, certain problems will resist solution, and certain predictions will not work out as the theory says they should. Adhoc modifications may be made to try and make the theory acceptable, but as the number of anomalies increases the theory will reach a crisis stage. During a

time of crisis rival theories will emerge and begin to receive serious consideration. Eventually some rival theory associated with a different paradigm will show itself capable of solving the anomalous problem, and will replace the older theory.

This is what Kuhn calls a scientific revolution. After such a revolution the new theory functions in a period of normal science and the whole process repeats itself. To understand this cycle, it is essential to understand what Kuhn means by a paradigm. Kuhn himself is not clear on this point and grants that there are different things that he has meant in different places. But in the broadest sense, a paradigm is a way of viewing nature, a point of view from which the world is seen, or a conceptual scheme and frame work.

Moreover we may identify three main functions of a paradigm one main function is to define and provide the language in which theory and observation will be stated by the scientific community, and to provide meaning for the terms of that language. Second a paradigm provides guide lines for the application of a theory, identifying those problems that

are the proper domain of the theory and the direction of future research. Finally, a paradigm provides some of the standards by which scientists will judge the success of the theory.

However the above description is the function of a paradigm. It does not tell us what a paradigm itself actually is. As far as the components of a paradigm are concerned, Kuhn says:

I shall identify three of these which, because central to the cognitive operation of the group, must particularly concern philosophers of science. Let me refer them as symbolic generalization, models and exemplars<sup>3</sup>.

We have already discussed exemplars in some detail and we understand some of the role play in giving a paradigm its definitional function. For the other two components, Kuhn says:

Symbolic generalizations, in particular, are those expressions, deployed without question by the group, which can readily be cast in logical form like  $(x) (y) (z) (x,y,z)$ . They are formal or the readily formalizable, components of the disciplinary matrix. Models ... are what provide the group with preferred analogies or when deeply held, with an ontology. At once extreme they are heuristic: the electric circuit may fruitfully be regarded as a steady-state hydrodynamic system, or a gas behaves like a collection of microscopic billiard balls in random motion. At the other, they are objects of metaphysical commitment: the heat of a body is the kinetic energy of its constituent particles, or more obviously metaphysical, all perceptible phenomena are due to the motion and interaction of qualitatively neutral atoms in the void<sup>4</sup>.

We should notice that disciplinary matrix is another word for paradigm. Kuhn says that symbolic generalizations are part of a paradigm. Kuhn does not see theory and paradigm as separable entities. The paradigm is the larger whole of which the theory is a part.

For logical positivism, symbolic generalization are the most important part of a theory. These generalizations are connected to observation statement. Kuhn, while acknowledging the role of symbolic generalizations in a paradigm, says that it serve as part of the definitional role played by a paradigm.

The way Kuhn uses the word model, it is not clear how models differ from exemplars. One possibility is that Kuhn takes a model to be something more general than an exemplar, so that exemplars are just one type of model.

Kuhn begins by offering a partial list of the sort of standards that scientists avail for theory choice. He says:



What, I ask to begin with, what are the characteristics of a good scientific theory? Among a number of quite usual answers, I select five, not because they are exhaustive, but because they are individually important and collectively sufficiently varied to indicate what is at stake. First, a theory should be accurate within its domain, that is, consequences deducible from a theory should be in demonstrated agreement with the result of existing experiments and observations. Second, a theory should be consistent, not only internally or with itself, but also with other currently accepted theories applicable to related aspects of nature. Third, it should have broad scope in particular, a theory's consequences should extend far beyond the particular observations, laws, or subtheories it was initially designed to

explain. Fourth, and closely related, it should be simple, bringing order to phenomena that in its absence would be individually isolated and, as a set, confused. Fifth, a somewhat less standard item, but one of special importance to actual scientific decisions - a theory should be fruitful of new research findings, it should, that is, disclose new phenomena or previously unnoted relationships among those already known<sup>5</sup>.

There is nothing privileged about the five standards Kuhn mentions here. This is one of his tenets that there are no privileged standards for theory choice. However, these five are illustrative of how a paradigm can determine standards of theory choice.

First, accuracy of predictions of a particular theory are not determined independent of a paradigm. When we judge the accuracy of a prediction, we are considering two things. First what result does the theory tell us to expect, and

second, how sensitive are the instruments by which the experimental result is being observed.

Scientists may have some theory independent methods of testing his theory of measurement. Galileo used a telescope to make astronomical observations, but he did not have to rely on only astronomical observation to tell him how accurate the telescope was as a measuring device. He could, for example, observe the motion of ships at sea to check the accuracy of his telescope.

Now, when a theory is initially proposed, the model on which the theory is based will constrain beliefs about accuracy. Later, as theory independent method of judging accuracy develop, these will have relevance only where the scientific community accepts certain exemplars for their theory measurement. So the exemplars and models of scientific community will not only determine how they judge the accuracy of a theory, but also how they judge the importance of accuracy. We can see this pattern repeated for any other criteria of theory choice also. Consider consistency which shall also follow the same general lesson that a scientific

community/ does not adopt external standards for theory choice. What criteria they use, how they judge the success of the theory relative to those criteria, and how they rank the importance of those criteria relative to one another will depend on the models and exemplars accepted by the scientific community. So the theory choice will be paradigm dependent. With this account of the Kuhnian paradigm, we may now explain in more detail the process of theory change of particular importance is the sequence of events Kuhn sees as characteristics of a scientific revolution, for it is out of this sequence of events that he sees the problem of incommensurability arising.

The emergence of a paradigm and the move into a period of normal science present a different picture.

By the time a theory has reached the crisis stage this has changed. After some time, certain problems will resist solution, and certain predictions will not work out as the theory says. Adhoc modifications may be made to try and make the theory fit the data. However, such adhoc modifications continually run the risk of violating the standards set for

what counts as an acceptable problem solution. Kuhn points out that a crisis stage will emerge.

At the crisis stage the scientific community begins to resemble a community in the pre-paradigmatic stage more than a community in a period of normal science for widening the standards for acceptability. The result is that the crisis period will reach a stage when not only rival theories, but also rival paradigm, may challenge the accepted theory and paradigm. The replacement of not just one theory by another, but one paradigm by another, is a scientific revolution.

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## CHAPTER IV

### KUHN'S NOTION OF INCOMMENSURABILITY

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A common understanding of the incommensurability thesis says that two different scientific theories can not be compared. But we need a more exact statement of this. There are two questions to answer in clarifying this thesis. First, what sorts of theories are we interested in comparing? Second what sort of comparisons are we interested in making?

Comparability: The notion of incommensurability here does not mean to make a comparison between any two theories. Such as, a comparison between socio-biology and quantum mechanics. We are concerned more particularly with theories that are about the same topic. For example we can make comparison of scientific theories proposed by Ptolemy, Copernicus, Kepler, Newton and Einstein because they talk about the same topic, namely celestial mechanics. Ptolemy, Copernicus, Kepler, Newton and Einstein also represent a historical sequence of predecessor and successor theories. Hence we shall be interested mainly in comparing two theories that are intuitively about the same topic, where one theory is the historical successor of the other theory.



There are, of course, many ways in which theories may be compared. We could compare the number of noble laureates who have advocated their theories or we may compare two political parties in our country. It is not these sort of comparisons we are interested in, nor it is these sort of comparisons Pohn says are impossible when he claims that theories are incommensurable.

In fact we are interested to accept a more rational theory on the basis of comparison out of two rival theories. We are concerned here only with those aspects of a theory which are relevant to scientific rationality. Thus to understand the incommensurability thesis we must understand what scientific rationality is? One problem is that there are as many views on what scientific rationality is as there are philosophers of science, but we shall attempt to provide here an overview of some of the main issues about scientific rationality.

Evaluating the goals of science exhibits additional difficulties. Does science have a single goal? If so, what would it be? We could say that the single goal of science is to provide a true picture of the world. This seems to be

inadequate. By simple inductive generalization we can conclude that no scientific theory has met this goal. All theories of the past have been rejected, therefore, the theories of the present may also be rejected. Providing a true picture of the world may be one of the goal of science. We learn nothing about how well theories so far have achieved the goal. If we say instead the goal of science is to provide theories which closely approximate the truth, then we face the difficult task of measuring approximation to the truth. And in any case all this assumes that any goal of science is to be characterized by truth. Let us suppose that we can agree on the goals of science, and agree on the standard of rationality. Given then the philosopher, like the scientist, must know how to gauge the passing landmarks on the way. We must know what sort of evidence we should expect in scientific inquiry, and what it signifies.

Kuhn's incommensurability thesis holds that those aspects of scientific theories relevant to deciding scientific rationality are not comparable. What this means depends on what we take scientific rationality to be. The preceding discussion is intended to show that whatever the answer to

this question might be, it will depend on three things: The goals of science, the methods of science, and the evidence available in science. Kuhn takes each scientific theory to be one part of a general paradigm supported by members of a scientific community. Thus Kuhn's thesis says that the goals of one paradigm are not comparable to the goals of another paradigm, or the methods of one paradigm are not comparable to the methods of another paradigm or the evidences available in one paradigm are not comparable in the evidences available in another paradigm.

Two further questions must be answered to clarify Kuhn's thesis. When Kuhn says that theories are incommensurable, does he mean that any two theories are incommensurable belonging to different paradigm or only that some theories are incommensurable within a paradigm? Second, is incommensurability always complete, or is that theories are partially incommensurable with one another. For first question Kuhn holds that theories are incommensurable because paradigms are incommensurable. Two paradigms do not share even goals, method and evidence of science. He seems to claim that whenever a scientific revolution occurs, the

theories before and after the revolution are incommensurable with one another. Hence, scientific theories across the paradigm are incommensurable. However, he never says whether or not theory change always requires a scientific revolution.

Moreover, some theories are not incommensurable because they are the part of paradigm. In answer to the second question, philosophers generally assume that Kuhn claims that when theories are incommensurable, they are completely incommensurable.

In The Structure of Scientific Revolution Kuhn holds that:

"Therefore, at times of revolution, when the normal scientific tradition changes, the scientist's perception of his environment must be re-educated in some familiar situations he must learn to see a new gestalt. After he has done so the world of his research will seem, here and there, incommensurable with the one he had inhabited before"<sup>1</sup>.

Thus we may take the final version of Kuhn's incommensurability thesis as:

For some pair of theories,  $T_1$  and  $T_2$ , such that  $T_1$  and  $T_2$  are about the same topic and where one of  $T_1$  and  $T_2$  is predecessor theory and the other is successor theory, there will be a paradigm associated with  $T_1$  and a different paradigm associated with  $T_2$  such that the goals of one paradigm will not be completely comparable to the goals of other paradigm, or the method of one paradigm will not be completely comparable to the goals other paradigm, or the evidence available in one paradigm will not be completely comparable to the evidence available in other paradigm?

What is the argument that supports such a thesis? To begin with, let us reconsider the logical positivist model of theory comparison and consider precisely how Kuhn would

argue against it. On Nagel's view, theory comparison is primarily a comparison of evidences. The goals and methods of science are not important. Given Nagel's account of theory comparison, it is clear that he holds that the goal of science is to provide true theories of the world. They achieve this goal to such an extent that they make more predictions. So according to Nagel, predictions are one of the main evidential bases for theory comparison.

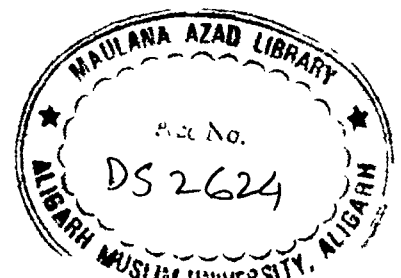
He identifies two types of theory reduction, homogeneous and heterogeneous. Homogeneous reduction applies when one theory is shown to be a branch of a broader theory. Because descriptive predicates are essentially the same between homogeneous theories, Nagel does not regard homogeneous theory reduction as problematic. It is the heterogeneous theory reduction, which shows disparity in descriptive predicates employed in the two theories to which he makes comparison.

As Nagel says that first the formal conditions must be met for the form of theory reduction. All of the axioms, special hypotheses and experimental laws of both theories must be

available as formulated statements. Furthermore, the meanings of all constituent terms of these axioms, special hypotheses, and experimental laws must be clear.

The structure of theories that Nagel proposes is typically positivistic, consisting of theoretical postulates, derived theorems from these postulates, laws governing experimental situation, correspondence rules for interpreting the theoretical postulates and theorems, and observational statements which constitute data to be explained. When the two theories are homogeneous, then one is reducible to the other if its observational results are derivable from other.

When theories are heterogeneous, then one must provide additional assumption that act as auxiliary correspondence rules not between the theoretical and observation statement but between the theoretical statements of each theory, for all statements in each theory that employ terms not occurring in the other theory. If the observational results of one theory are derivable from the observational results of the other, then the former theory is reducible to the latter.



Even on the basis of what we have presented on Kuhn's position earlier, there are several objections Kuhn would raise to Nagel's view. He might first of all deny that all the axioms, special hypotheses, and experimental laws of each theory are explicitly statable. If they are not, then Nagel's necessary conditions have not been met, and theory reduction is not possible. Perhaps all of these elements are part of what Kuhn calls the symbolic generalization of a paradigm, and are in fact explicitly stated. Nagel further requires that the meanings of all the constituent terms be fixed and Kuhn would deny that they are. The meaning of both theoretical and observational terms are determined in part not by definitions, but by exemplars. Exemplars are not explicitly statable, nor are they interpreted in the same way by all scientists even of a given scientific community hence the meaning of terms is not absolutely fixed. Thus Kuhn would deny that the necessary conditions to check for derivability of observation statements are fulfilled.

In addition, Kuhn would question whether any apparent pairs of homogeneous theories are in fact homogeneous. Kuhn continually points out that just because Newtonian and



Einsteinian mechanics both employ the term 'mass' does not mean that the same concept or 'mass' is employed by both theories. The mere fact that two theories employ the same descriptive terms does not show that they employ the same descriptive concepts for instance 'mass' is absolute and relative term in Newtonian and Einsteinian mechanics respectively and hence does not show that they are homogeneous.

Finally Fuhn would deny that we can make the assumptions that would be necessary to bridge theoretical terms in one theory to the theoretical terms in another theory. It is Fuhn's reasoning on this point that is most important and provides the key to understand how he could criticize just a particular account of theory comparison.

Translatibility: We have already seen several important claims that Fuhn has made. For the cases we are concerned with, two different theories will be part of two different paradigm. Consequently, they will be stated in two different languages. Even the observational languages of theories will not be the same. Prima facie, the problem of theory

comparison should be understood in terms of problem of translation. We may call it translatability. If there will be a theory neutral observation language, then the problem of translation might not be a problem at all. But we have already seen powerful arguments from contextualists such as Fuhn, that there is no such theory neutral observation language. We should now to consider what we mean by a theory-neutral language. We mean a language that is neutral with respect to all theories and shares by all theories. There are two questions. First, can translation be done directly with appeal to the medium of some third shared language? This is, how we translate Urdu into English, English into Hindi etc. Second, if the medium of some shared language is needed, must it be neutral with respect to all theories or two theories to be compared? There is no theory neutral observation language in general it does not immediately follow that there is no language which is neutral to the theories to be compared.

Why is it that we can make direct translation between Urdu and English? We can directly translate because though Urdu and English use different terms, and even different

grammers, they do not use different concepts. We consider then different languages but not different conceptual schemes. Different paradigms are not like different languages but states:

In a sense that I am unable to explicate further, the proponents of competing paradigms practise their trades in different worlds. One contains constrained bodies that fall slowly, the other pendulums that repeat their motions again and again. In one, solutions are compounds, in the other mixtures. One is embedded in a flat, the other in a curved, matrix of space. Practising in different worlds, the two groups of scientists see different things when they look from the same point in the same direction. Again, that is not to say that they can see anything they please. Both are looking at the world, and what they look at has not changed. But in some

areas they see different things, and they see them in different relations one to the other.<sup>2</sup>

What Kuhn can possibly mean here when he claims that scientists in different paradigms practice in different frame works and yet that they are talking about the same world and hence different paradigms represent different conceptual schemes. The problem of translation is not just a problem of determining the relation between the terms and concepts. Each theory uses concepts that are unique to its paradigm. Consequently translation will have to be done through what is shared between the two paradigms. This shared thing is precisely what we mean when we speak of a third, neutral language. Thus direct translation will not work.

Is there a language which is neutral with respect to the two theories which suffice to make theory comparison possible? We must consider what we mean by neutral language. 'Neutral' is a valuative term, and we must ask neutral with respect to what?

A theory neutral observation language means a language which is epistemically neutral. In other words, a theory-neutral observation language would be one whose statements are not interpreted, they are mere factual reportings.

We must also recognise that in giving up a general theory-neutral observation language we are making an epistemic concession to the theories. If the results of two theories can each be restated in a general neutral observational language, we are getting then the factual content of each theory. Hence we compare the theories against an established epistemic standard our judgement then that one theory is superior the other.

Can there be a neutral language that serves as a medium for translation between two paradigms? Kuhn replies in negative and to understand his reasons we must understand his metaphor in saying that scientists in different paradigm practise in different worlds which is different conceptual frameworks.

The problem is not just one of translating between languages that do not share all the same concepts. The problem is that

the meaning of some of the divergent concepts is understood by exemplar not by definition and hence formal translation procedures that operate on stutable statements do not work.

As Luhm says:

Something like a paradigm is pre-requisite to perception itself. What a man sees depends both upon what he looks at and also upon what his previous visual-conceptual experience has taught him to seem. In the absence of such training there can only be, in William James's phrase, "a bloomin', buzzin', confusion"<sup>7</sup>.

It is in the sense that an understanding of the world by a scientist depends upon the paradigm in which he works.

Theories stated in different paradigms are not translatable in the following senses. They are not directly translatable from one language to another, as we translate Urdu to

English. Moreover, they are not translatable through the medium of some third language which is common to both paradigms. Translation of these kinds fails because a paradigm is not just a language, it is a conceptual scheme. To say that a scientist belongs to a paradigm is not just to say that he shares with other scientists of his community a common language of science, but that he shares a common way of viewing the world which is unique to that of paradigm. A conceptual scheme is not based on techniques for interpreting experience, but on that fact some fully conceptualized experiences are direct, and not interpreted or inferred. It is the ability to see a table, and not just infer that a brown rectangular smudge is a table, that makes language in general and hence any particular language possible. In the language of science statements are of higher complexity than "That is a table" that have this basic and direct character. In science it may be statements such as 'that is an electron' or 'that is a photon'. That are equally direct for a particular scientist. No inference occurs. It is simply part of how he has learned the language of his science that sanctions such assertions in the right context. One could ask, 'How do you know that is Photon?'

and perhaps elicit some justification from the scientists as a response. But the thrust of Luhn's argument is that the question need not elicit such a response. There will come a point when the scientist will not regard your question as a proper question of science. His only response then is more likely to be something like 'Go and take some physics courses. These are the points in a paradigm where one has reached the limit of the statable, where what remains is so basic that no explanation can be given.

Logical positivistic account of theory reduction relies on comparing empirical results that are derivable from each of the theories to be compared such an account fails for two reasons. First, certain formal conditions about stability and fixed meaning of terms fail to be fulfilled. Second, to make a comparison of derived empirical results, we must be able to determine which theoretical statements in each theory are to be compared, and we must be able to translate from one theory to another, and by Luhn's arguments this cannot be done.

Goals and Methods: Consequently, much of the discussion of Luhn's incommensurability thesis centres on how to solve a



perceived problem of translation. Nevertheless, the incommensurability thesis touches much deeper issues, and translatability is only one problem. What is the value of derived empirical results, even if they were comparable? Evidence for a scientific theory is one of the elements we must consider in an account of scientific rationality, and it is in a sense, a secondary element. We can judge the worth of evidence if we know the goals to be achieved, and the means to be used to achieve those goals. Here we see another problem. Nagel like other scientific empiricists, assumes that scientific theories share a common goal and common method, thus reducing the problem of rationality to the problem of comparing evidence. But it is also part of Fuhn's thesis that the accepted goals, and accepted method, are not universal in science but paradigm dependent.

To explain how methods and goals between paradigms may be not comparable. Let us consider the development of celestial mechanics at the time of copernicus and review briefly the state of ptolemaic astronomy prior to him.

The accepted view from the time of Aristotle until the time of copernicus was that the Earth stood at the center of the

universe, and that all other celestial bodies orbited the Earth in circular paths, with the planet nearest the Earth, and the stars on a more remote celestial orbit. The two most pressing problems faced by ancient astronomers were to provide a celestial basis for a working calendar and to provide for a celestial navigation.<sup>4</sup> Towards these ends it was necessary to provide a framework in which the locations of the stars and planets could be predicted. However, prediction was a secondary goal; it was important only as a means to the larger ends of constructing a calendar and conducting celestial navigation.

The two most important celestial bodies for devising a calendar were the sun and the moon. The completion of one cycle by the sun around the earth was taken as the standard for a calendar year, and the completion of one Lunar cycle was taken as the standard for one calendar month. The problem was to reconcile the moon's irregular cycle with that of the sun, to be able to relate months to years systematically. There were difficulties:

The moon travels around the ecliptic faster and less steadily than the sun. On the average it completes one journey through the Zodiac in  $27\frac{1}{3}$  days, but the time required for any single journey may differ from the average by as much as 7 hours.<sup>5</sup>

The lunar cycle reconciling month and year would not have been problem for ancient astronomers if the length of the moon's cycle could be predicted with any regularity. However:

Successive new moons may be separated by intervals of either 29 or 30 days, and only a complex mathematical theory, demanding generation of systematic observation and study, can determine the length of a specified future month.<sup>6</sup>

Thus much of the work of early astronomers was to compile a generations old record of the moon to serve as the basis for the required mathematical theory. It took astronomers literally thousands of years to devise a workable calendar.

Astronomers attempting to predict the movement of the planets faced another difficulty. Even the planet's apparent motion does not fit into the model of circular orbit around the earth. Periodically, each planet would go into a period of retrograde motion. It became necessary to postulate a more complex mechanism for the motion of planets that there was a point on a circle which orbited the earth and that the planet actually orbit about this point on a smaller circle. This circle was called the epicycle. The most of the works of Ptolemy to Copernicus was devoted to get some network of circles that would perfectly match observed motion of the planets. Their efforts never fully succeeded:

For its subtlety, flexibility, complexity, and power, the epicycle deferent technique ... has no parallel in the history of science until quite recent times. In its most developed form the systems of compounded circles was an astounding achievement. But it never quite worked. Apollonius' initial conception solved the primary planetary irregularities - retrograde motion,

variation of brightness, alteration in the time required for successive journeys around the ecliptic and it did so simply and at a stroke. But it also disclosed some residual secondary irregularities. Some of these were explained away by the more elaborate system of compounded circles developed by Hipparchus, but still the theory did not quite match the result of observation. Even Ptolemy's complex combination of deferents, eccentrics, epicycles, and equantes did not precisely reconcile theory and observation, and Ptolemy's was neither the most complex nor the last version of the system. Ptolemy's many successors took up the problem where he had left it and sought in vain for the solution that had evaded him. Copernicus was still grappling with the same problem.<sup>7</sup>

Planetary astronomy represented a unique period in the history of science. It is perhaps the only time in the history of science when detailed observation had been made over a period of centuries without producing any startling new results. New observations were only more accurate confirmations of older observations. What is important about this point is that by the sixteenth century, the practical need for a theory of celestial motion had vanished. No mathematical theory was required at all to determine how long the next lunar cycle would be, or where venus would appear in the sky on the following winter solstics. Anyone who desired such information needed only to consult the tomes of tables that had been compiled. In this sense, then, astronomers who continued to work on the Ptolemaic theory were not attempting to provide a theory of the unknown, but a plausible history of what was already known.

Copernicus's discovery was to abolish the whole system of epicycles and deference and restore simple circular motion as the motion of the planets. This was done by postulating that it is the sun, and not the earth, that is at the

centre, and that the earth both rotates on its axis and orbits the sun. Today this is perceived as the most revolutionary of Copernicus's achievement. In fact what motivated Copernicus to adopt this discovery was that he could offer a completely different account of retrograde motion which was not an actual motion of the planets, but an apparent motion perceived because the earth and other planets move at different velocities and at different distances from the sun. What is clear from the history of the time is that the edge, Copernicus had over Ptolemaic system was his solution to the problem of retrograde motion. Astronomers came to regard to his solution to the problem as a better solution. What is difficult to explain is why they regarded it as a better solution, and why they regarded solution of the problem of retrograde motion as so important.

Kuhn would say that those who accepted the Ptolemaic paradigm had different goals or different views about the importance of various goals, and that they accepted different methods from those who accepted the Copernicus paradigm. He would further claim that the goals and methods of the two paradigms were not entirely comparable.

Kuhn continually speaks of the scientist finding his new paradigm incommensurable with his old or of scientists from different paradigms facing incommensurability of their views. While incommensurability of this sort is undeniable. It is quite a different thing to claim that no one philosopher or historian can make the relevant cross-paradigm comparisons. There is good reason to think that comparing evidence for a theory associated with one paradigm to evidence for a theory associated with a different paradigm is a problem even for the philosopher, precisely because of the severe linguistic difficulties. But it is by no means clear that the same barriers to comparison exist when it is the goals or the methods of theories in different paradigm we wish to compare. Let us consider one historical thesis about the Copernican revolution. Prior to Copernicus, the goals of astronomy as a science were bound to theological and metaphysical ideals. Apart from the practical goals of navigation and resolving the calendar, there was a desire on the part of of the Catholic church to advocate a certain view about celestial objects that treated them not as mere physical objects, but as heavenly objects.



Furthermore, as a consequence of certain theological doctrines, it was necessary that the heavens be viewed as finite and bounded, and that celestial objects have a certain geometrical and mathematical perfection to them. All of these doctrines found their place in Ptolemaic system because of the scholastic interpretation of Aristotle.

If we try to classify the events surrounding the copernican revolution in Kuhn's scheme for scientific revolutions, then we would place the publication of Copernicus's De Revolutionibus Orbium Caelestium in 1543 at the peak of a crisis stage for the Ptolemaic paradigm. The publication of Copernicus' work represents the point at which sufficient doubt has been raised about the Ptolemaic paradigm to allow as competitors not just rival Ptolemaic theories but rival paradigms as well. The period of revolution is effectively closed, and a new paradigm and new period of normal science begun, with Kepler's development of the three laws of planetary motion. Consequently, those astronomers living in the time between Copernicus and Kepler are a complex of new and old ideals about the goals of astronomy and its methodology.

Perhaps more importantly, Copernican astronomers no longer felt the practical need to present an astronomical theory of great accuracy. Celestial navigation and work on the calendar would continue independent of the astronomical theory, operating on the strength of centuries long compilation of observations.

Finally, and most important of all, Copernican astronomers rejected the Ptolemaic methodology, which required all theorizing to be done within the constraints of a system of epicycles. Kepler did it successfully, but he was not the first to experiment. The Copernican approach was that it opened up new research possibilities in a field that had been dead for centuries. The astronomers weighing the Ptolemaic against the Copernican solution to the problem of retrograde motion was likely to be motivated primarily by this consideration: accepting the Copernican solution showed the way to creative and innovative attempts to theorize about astronomy. One historical theory about the Copernican revolution might be summarized as follows:

Post-copernican astronomers either did not show the scholastic interpretation of Aristotle as the only way to preserve the heavenly status of celestial bodies or eventually abandoned considering celestial bodies as heavenly and treated them as physical objects. Further, they acknowledged that the practical needs for a theory of astronomy had changed, and that accuracy was less pressing as a goal. Apart from these differences in goals with their Ptolemaic counter-parts they also accepted a different methodology. Rather than follow a system that required a set of only circular motions, a system where genuine opportunities for creativity had been dead for centuries, they accepted a more liberal view about the geometrical forms that could appropriately be applied to astronomy. But this does not make incommensurability about goals and methods a general, or even a philosophical problem. The historical thesis, we have presented which is a very direct attempt to compare goals and methods of two different paradigms, could well be true. Further, its truth depends not on philosophical matters, but on historical matters. No philosophical issues need be resolved for us to evaluate the historical thesis, its resolution would depend upon precisely the sort of debate that surrounds any historical work. The questions are all empirical, not philosophical.

There is a Kuhn's argument that goals and methods of one paradigm are not comparable with goals and methods of another. It is one thing to point out that Ptolemaic astronomers treated celestial objects as heavenly bodies, whereas astronomers after Kepler by and large treated them as mere physical objects.

It is quite another thing to say that astronomers should have been treated or should now treat celestial objects as mere physical objects. We are inclined to judge that it is Copernicus and not Ptolemaic who held the rational belief in such matters. However we feel that the history of science has proved Copernicus right and Ptolemaic wrong.

The discussion at the end of this Chapter is meant to illustrate that there is more to scientific rationality than simply getting the right answer. We do not think that Copernicus was simply lucky, we think that he had good reasons for his claim.

To give a satisfactory comparison of the methods and goals of different paradigms, we would need standards of good

reasons that can be generalised across historical context. Kuhn's argument on this point takes a form of challenge. He does not offer an argument to show that there can not be any such standards. Rather he has issued the challenge to philosophers to produce such standards. In their absence, goals and methods can be judged only relative to the paradigm in which they occur. Comparison in the form of comparison of their merit is impossible. The best that we can say is that each paradigm has the goals and methods independently.

Progress: A final point worth considering is that, in the face of his own arguments, how does Kuhn defend the claim that science does in fact progress? This is, after all, a thesis he is committed to:

Why should the enterprise sketched above move steadily ahead in ways that, say, art, political theory, or philosophy does not? Why is progress a pre-requisite reserved almost exclusively for the activities we call science? The most usual answers to that question have been denied in the body of this essay.<sup>8</sup>

Kuhn's first answer is that, in a sense, there is no answer. He regards the assertion that science progress is tautologous. Science is an exemplar for progress, it is paradigmatic of what progress is:

Part of the question is entirely semantic. To a very great extent the term "Science" is reserved for fields that do progress in obvious ways.<sup>9</sup>

Kuhn suggests that our motivation for wanting to define progress, or define science, is not that the definitions themselves are so important:

Inevitably, one suspects that the issue is more fundamental. Probably, questions like the following are really being asked. Why does my field fail to move ahead in the way that, say, physics does? What changes in technique or method or ideology would enable it to do so? These are not, however, questions that could respond to

an agreement on definition. Furthermore, if precedent from the natural sciences serves, they will cease to be a source of concern not when a definition is found, but when the groups that now doubt their own status achieve consensus about their past and present accomplishments.<sup>10</sup>

While it is certainly true that practitioners of a discipline cease to worry about what science or progress is once they have settled that their own discipline is in fact a science and does in fact progress, this is not reason to claim that the questions that concern practitioners could not respond to an agreement on definition. If we can understand what connects one paradigm its successors, what is preserved and carried on from one period in the history of science to the next, we will not only be reassured about the status of clear sciences such as physics and chemistry, but will be able to comment on less secure disciplines such as Psychology or Sociology, where instead the philosopher of Science must now all too often remain silent. We might expect Tuhn to argue that Science progresses just because

that is the sort of thing it is, but the Philosopher should not be happy to settle for such a answer. In one sense Kuhn's notion of progress is characterised by the revolution which takes place in paradigm shift. This is contrast to the logical positivists on account of progress in term of accumulation of scientific knowledge. Hence, in Kuhnian terms, revolution is the indicator of scientific growth which in positivistic framework accumulation is the indicator of growth in Science.

In conclusion, we say that Kuhn's argument for incommensurability has two main points. First, that evidence for a theory associated with one paradigm is not fully comparable with evidence for a theory associated with another paradigm. The meaning of term is dependent in part on exemplars and models, and hence part of the meaning of a theory rests on an inarticulable basis. Thus translation cannot be done directly. It would have be done through the medium of some mutually shared concepts. However, different paradigms are not only different in language, but different in the nature of radical conceptual differences between paradigms. Hence, it is not fully,



large set of mutually shared concepts to enable even an indirect translation. Second while the goals and methods of one paradigm can be compared to the goals and methods of another paradigm, at least to the extent that any historical comparisons are possible, there is a special problem for making these comparisons as a basis for judgement about scientific progress.

It is not enough to be able to identify what the goals and methods of each paradigm were, and to note how they differed. To determine scientific progress we must also be able to make an evaluation of these goals and methods. Kuhn challenges philosophers to produce epistemic standards that can be generalized across historical contexts in order to make such evaluations.

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## CHAPTER V

## CONCLUSION

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### CONCLUSION

Philosophers are generally concerned with understanding of human knowledge. Scientific knowledge is the best example of human knowledge. Thus on the one hand, if any branch of human knowledge is safe from charges of relativism or subjectivism, it should be scientific knowledge and on the other hand, if we want to understand that makes any other branch of human knowledge objective, we should look first to the model of scientific knowledge. This is what many philosophers of science think.

In chapter two, we took up the task of understanding the nature of scientific theories according to philosophers of science in the positivist tradition which gives us an account of its approach to science. The proposed approach to science never succeeded in explaining the meaning of theoretical terms as not meeting the necessary conditions for objective knowledge claims. To do so would further require revision of criteria of meaning and a richer account of observation statements.

However, we see that some positivists, like Hempel were in favour of to allow non-empirical statements to play some role in science when conjoined with theories that have empirical content. There is also recognition of the fact that theoretical terms are not and should not be amenable to complete, explicit definition. There is also some indication that Hempel and Reichenbach had an appreciation for the holistic aspect of the language of science.

An analysis of observation statements was not expected from logical positivism because there seem to be no need for such an analysis. The dynamics of theory change simply was not an issue. Only, Nagel addresses the concept of scientific progress and his analysis focusses on the narrow concept of theory reduction.

One may classify these problems into linguistic and epistemic concerns. Logical positivists had to accept that a full analysis of the meaning of theoretical terms required a set of explicit definition.

The epistemic status of scientific theories according to logical positivism is simply one large question mark. It is clear in the work of Carnap and others and in the work of Nagel that how the merit of a theory will depend upon the observational consequences of the theory. This view however, tells us nothing about the ultimate status of observation statements. On this latter point logical positivism is simply silent. It assumes that observation statements constitute a privileged class that are self justifying, but this position is never actually stated. Further the only account of commensurability depends upon the assumption that the observation language is fixed and reidentifiable from one theory to the next.

In Chapter three we began to explain Kuhn's criticism on positivism philosophy of science. Two important themes are introduced. The first theme challenges the positivist account of theories and provides an account of meaning. The second theme provides dynamic of theory change.

According to logical positivism, a scientific theory consists of a set of symbolic generalization, tied to

predictive consequences by stutable correspondence rules. The theoretical terms utilized in these generalization can be defined by set of definitions. Predictive consequences and definitions will be by and large in observational terms. This structure does not provide neither a full account of the language of science nor a full account of the epistemic basis of science.

Luhn held that definitions on infinite regress or circularity must come to an end because it falls short of giving theoretical terms full meaning and the epistemic status of theories cannot depend upon unexplained status of observational statements. Thus a theory must be treated not just an abstract entity but also as one part of a larger network of shared doctrine by a scientific community.

It seems clear that logical positivists expected that theory comparison to be an issue of cumulativeness of scientific knowledge or showing how older theories are subsumed as special cases of their successors or atleast showing how all predictive results of older theories can be reproduced by their successors. Luhn, however is adamant in arguing that

development of science does not follow this building block model. He sketches a picture of the history of science as a series of discontinuous jumps, where one paradigm and the embedded theory associated with it, is replaced fully by another paradigm. As far as the question of comparison of scientific theories is concerned, Kuhn held that a change in paradigm means also a change in language. He does argue that theories are incommensurable and thus non-comparable.

In Chapter four, we presented Kuhn's argument. The primary question of this chapter is what is the basis of saying that theories cannot be compared. Kuhn argues that the meaning of scientific terms of theories across the paradigm are distinct and different paradigm not only use different language but also belongs to different conceptual schemes. This is a general reason that he claims that we cannot talk of the empirical result of one theory being reducible to be empirical result of another.

As far as goals and methods of paradigm are concerned, Kuhn held that there is no common standard for comparing them and hence each paradigm has the goals and method independently.



In nut shell, Kuhn argued that established theory reaches a point of crisis where numerous anomalies are not resolveable by it. Consequently various rival theories get attention and are considered seriously which may have been lurking in the back-ground for sometime. These theories can not be compared because they use terms in different meanings and hence theories are incommensurable. One of the rival theories overthrows the current theory when it resolve those anomalies more successfully. His views on non-comparability and non-translatability are directly linked to his notion of incommensurability.

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